



Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

THE PRESENT STATE OF THE THEORY OF DISTRIBUTION

JACOB H. HOLLANDER

The theory of distribution has been the primary endeavor of economic inquiry during the largest part of its systematic pursuit. Philosophical interest and practical affairs have alike stimulated the search. Obviously a theory of economic apportionment must figure in any analysis of social relations, and the economic problems of each generation have emphasized the need of theoretical standards.

The physiocratic concept of economic inquiry was essentially the search for a scheme of economic partition. Recent historical studies have made clear that Adam Smith "acquired the idea of the necessity of a scheme of distribution from the physiocrats, and that he tacked his own scheme (very different from theirs) on to his already existing theory of prices."¹

But it was pre-eminently the practical economic controversies of the early nineteenth century that brought the theory of distribution into prominence. The currency debate was at bottom an issue between the tax-payer and the fund holder, just as the corn-law controversy hinged on the question as to whether the commercial policy of England should be shaped in the interest of the agriculturist or the manufacturer. Manifestly no wise counsel could be given the legislator until the principles which governed the shares of the respective claimants were understood. In 1817 Ricardo wrote in the preface to the first edition of his "Principles,"

¹ Edwin Cannan, (ed.) "Lectures on justice, police, revenue and arms, delivered in the University of Glasgow by Adam Smith," (Oxford, 1896), xxxi.

"To determine the laws which regulate this distribution, is the principal problem in political economy ;" and a little later to Malthus, "the laws which determine the division of the product of industry among the classes who concur in its formation" constitute "the true laws of our science."¹

The didactic quality of the later Ricardians restored a balance, and with McCulloch and the elder Mill, the theory of distribution took rank again as the co-ordinate but not exclusive endeavor of economic inquiry. Influenced by the socialistic assault upon the competitive system and the alternate schemes of distribution which the socialists pressed, John Stuart Mill in 1848 drew a distinction between "the laws and conditions of the production of wealth, [which] partake of the character of physical truths," and the distribution of wealth which is "a matter of human institution, solely."² But the distinction affected the political economist as publicist rather than as scientist. It meant much to the legislator whether the existing distributive system was an expression of natural law or of artificial convention. But to the investigator its data demanded interpretation in any event.

By the early sixties the theory of distribution had attained a fairly definitive and compact form. The principles of the wage-fund in industry, diminishing returns in agriculture, and abstinence in capital formation had been developed into respective "laws", and if these lacked the harmony or unity of a true synthesis, they at least afforded epigrammatic dicta and convenient

¹James Bonar (ed.), "Letters of David Ricardo to Thomas Robert Malthus, 1810-1823" (Oxford, 1887), p. 175.

²"Principles of political economy, with some of their applications to social philosophy" (London, 1848), Book II, chap. 1, § 1.

rules of thumb of highest effectiveness, for the settlement of the practical controversies of the period.

The wage-fund formula—the key-stone of this classical theory of distribution—held sway, with at best inarticulate and unavailing dissent for the two generations following Ricardo. In the decade from 1866 to 1875 the inevitable reaction came. Longe's indictment in 1866 was followed by Cliffe-Leslie's criticism in 1868, by Thornton's attack in 1869, and by Mill's ponderous recantation in the same year. Whatever new life was given by "patched and revamped versions" thereafter, was formally extinguished by the powerful assault of our soldier economist, Francis A. Walker, in 1875-1876.¹

Mill's recantation marks—it might be said was largely responsible for—the breaking of the spell which the determinism of the wage-fund had cast upon economic thought. Thenceforth, the critical contributions of Cairnes, Jevons, Toynbee, Hearn, and Walker—crystallized about a favorable nucleus. In 1878 Ingram delivered his sensational address on "The present position and prospects of political economy". In 1879 Cliffe-Leslie's "Essays in political and moral philosophy" was made accessible, and in the same year Marshall put together the substance of his Cambridge lectures in the "Economics of industry". In 1880, Arnold Toynbee complained "though the wage-fund theory has been given up by economists, it is extremely difficult to frame another theory in its place which shall explain the facts,"² but Sidgwick's criticism had already

¹ See two papers by the present writer, "Political economy and The labor question" in *North American Review*, April, 1903, and "The residual claimant theory of distribution" in *Quarterly Journal of Economics*, February, 1903.

² "Wages and natural law" in "Lectures on the industrial revolution" (London, 1884), p. 159.

stimulated a brilliant American economist to develop his theory of wages into a "complete and consistent body of doctrine respecting the distribution of wealth," and first in his "Political economy" in 1883, and repeatedly thereafter, Walker presented the differential theory of profit and the residual theory of wages.¹

The boldness and the originality of Walker's generalizations would have provoked criticism at any time. But coming as they did at a juncture when a coincidence of economic students and economic issues had effected a renaissance in economic study in the United States, when "the extreme *Historismus*" of the German school had stirred up reaction in favor of critical speculation in Austria, in England, and in America, and when the single-tax agitation, the new importance of the entrepreneur, and the extension of corporate organization to industry had directed urgent attention to the practical problems of distribution—the result was a period of intense controversial activity to which the early volumes of the *Quarterly Journal of Economics* and the first sessions of the American Economic Association bear impressive testimony. Before the fire had even begun to smolder, the flames were again stirred into furious activity by the presentation in English dress, through Bonar's exposition and Smart's translation, of the more important views of the newer Austrian economists. At the same time, Clark presented in rounded form a theory of imputation of productivity, outlined nearly a decade before, and in 1890 Marshall fulfilled the promise of his earlier studies by a clear and attractive synthesis.

Since 1890 criticism and speculation have been de-

¹Cf. "The residual claimant theory of distribution" in *Quarterly Journal of Economics*, February, 1903.

voted in increasing amount and intentness to the theory of distribution as so presented. The present participants in the fray include no mean proportion of contemporary economists. To mention only those of England and the United States,—Clark, Giddings, Patten, Wood, Commons, Macfarlane, Carver, Smart, Hobson, Fisher, Cannan, and Johnson have made specific contributions, while Bullock, Hadley, Davenport, Fetter, Seager, Flux, and Seligman have inserted more or less independent theories of distribution in the text book exposition of economic principles.

It is not an inspiring contrast to turn from this record of sustained activity to either its net result or its contemporary phase. It appears that for nearly a century the theory of distribution has been the centre of economic inquiry. And yet no doctrine nor series of doctrines has been formulated that by the most optimistic stretch could be termed an interpretation of modern industrial distribution. The very phrase "the present state of the theory of distribution" is misleading to the extent that it intimates any unanimity or agreement. The reviewer turns in depression from his task persuaded—to paraphrase Renan's epigram—that theories of distribution are as vain as religions and philosophies even though the theory of distribution is as real as philosophy or religion.

The history of science—and pre-eminently of economic science—warns that a period of unanimity is not necessarily one of achievement. The famous surgeon, Baron Boyer, declared over a hundred years ago, when anaesthesia was undiscovered and infection of wounds was not understood and could not be prevented, that surgery had then reached almost, if not actually, the highest

degree of perfection of which it was capable.¹ Similarly, McCulloch wrote in 1824, "the errors with which political economy was formerly infected have now nearly dissappeared",² and John Stuart Mill's classic dictum in 1848 of the then perfection of the theory of value needs no re-statement before this body.³ Conversely, a stage of doctrinal unrest is not necessarily one of stagnation. The daze and uncertainty to which recent discoveries have reduced modern physico-chemical study will certainly not hereafter be accounted an unprofitable episode, and no period of classical political economy was so fruitful as the decade from 1815 to 1825 when active economic thinkers of the day—Malthus, Ricardo, James Mill, McCulloch, Torrens, and Senior—were arrayed in doctrinal antagonism, and when Sydney Smith is said to have joined the Political Economy Club to learn about the mysterious theory of value, only to resign promptly thereafter because the membership knew no more of it than himself.

It is not so much, therefore, the lack of identical conclusion as the absence of recognized criteria of scientific progress that casts discredit upon the theory of distribution as now current. If recourse be had to the readiest empirical measure—public estimate—we are left in no manner of doubt that the theory of distribution as tested by the practicability of its application, is gravely deficient. For example, at the present moment there are three great economic problems disturbing the con-

¹ Dr. William S. Halsted, "The training of the surgeon," in *Johns Hopkins University Bulletin*, vol. xv, no. 162 (September, 1904).

² "A discourse on the rise, progress, peculiar objects, and importance of political economy." (Edinburgh, 1824), p. 9.

³ "Principles of political economy." (London, 1848), Bk. III, ch. I, § 1.

sciousness of the American people: trade unionism, the growth of corporations, and socialism. It should be as natural and proper for the public mind to turn to the scientific economist for specific and definite guidance with regard thereto as for the cotton planter to hearken to the zoologist in planning a campaign against the boll-weevil, or for a municipal administration to turn to the pathologist for council in guarding against epidemic typhoid. Each of the three problems can be simplified, if not solved, by the determination of an underlying principle of distribution. The *crux* of trade unionism is the recognition of a natural law of wages, and, no less important, a practicable method of ascertaining it. The corporation problem will speedily enter upon a new phase if the character of entrepreneurs' gains be understood. The public will know how to deal with socialism when the functions of capital and the law of interest are grasped. In each of these directions, the economist might properly be expected to meet, indeed to anticipate the public's appeal for counsel; and in each of these directions, the economist within the ken of the ordinary man of affairs is mute.

But, more fundamentally, the theory of distribution as now current is in an unpromising condition. In 1885 Sidgwick replying to Ingram's challenge declined to recognize sociology as an established science because it failed to satisfy the two simple tests formulated by Auguste Comte of the real establishment of a science:—1st, Consensus or continuity; and 2d, prevision. "When we find", Sidgwick stated in Comte's own words, "that recent works instead of being the result and development of what has gone before, have a character as personal as that of their authors, and bring the most fundamental ideas into question—

then we may be sure we are not dealing with any doctrines deserving the name of positive sciences.”¹

It would be difficult to phrase a more accurate characterization of the present state of the theory of distribution. The most recent works on the subject have certainly a character as personal as that of their authors, and no self-respecting writer on distribution in the past decade has failed to bring the most fundamental ideas upon the subject into question.

If the validity of Comte's criteria be accepted, it is impossible to escape the logic of his syllogism or to resist the conclusion that the theory of distribution in its present phase is not and gives little promise of becoming, a positive scientific doctrine. Certainly adequate occasion seems to exist here for a close examination of the state and tendency of the doctrine.

At the very outset of such a review a fundamental query presents itself. Is the palpable deficiency in the theory of distribution one of result or of method? Is the economist on the right track in his search for a principle, but compelled to move slowly and cautiously because the country is broken and the obstacles are myriad? Or has he utterly lost his trail, and is now floundering around aimlessly after a mere will-o'-the-wisp, with the certainty that sooner or later he will have to retrace his foot-steps and start anew? In other words, of the several methods appropriate, singly or in combination, to economic inquiry, is the modern economist using, in the search for a theory of distribution, that one or that combination best suited to its scientific determination? *If he is, then let us take new heart of grace, make firmer resolve and assail the problem with renewed*

¹“The scope and method of economic science,” reprinted in “Miscellaneous essays and addresses.” (London, 1904), p. 193.

energy. If he is not, let us recognize the error and seek to right the misdirection of effort.

It requires the barest glance to realize a startling contrast in method between the search for a theory of distribution and the pursuit of any ordinary principle of positive science. Let us turn for a moment to chemistry, where within recent years the bounds of organized knowledge have been extended with the most brilliant results. In so far as the layman may speak, it appears that modern chemical—or for that matter, physical or biological—study involves three consecutive stages: (1) observation and experiment, (2) theorization, (3) verification. Associated with these essential activities are the contemporary processes of initial conjecture affording a tentative working plan; formation of trial hypotheses in consequence of investigation and for submission to experiment; and conversion of theory by test into law. But in the main chemical science advances from truth to truth, from probability to certainty, because a body of mature workers, equipped with intimate knowledge of the achieved, are busy marshalling and classifying facts, searching for and formulating uniformities, testing hypotheses, and demonstrating laws.

If we turn now to the scene of economic study and to the theory of economic distribution, the contrast is fairly startling. We find a body of capable and devoted workers, and a definite and inviting subject-matter. But here, to any appreciable degree, the parallelism stops. There is no collecting and classifying data in their qualitative aspects, no tentative selection of economic uniformities, no verification of hypotheses by reference and comparison. As against the chemical investigator in his laboratory, deliberately and systematically gathering a particular group of facts, and formal-

ly submitting the sequences which they suggest to comparison and to test, with a reasonably well established hypothesis as the ultimate endeavor, we have a corps of student apprentices busy upon historical and institutional monographs; a group of younger scientists absorbed in academic duties and text book writing, and a body of sages engrossed in doctrinal dialectics. A single category has rarely been used to include two things less identical than the term "scientific" in reference to these two activities. If the one be, the other is not. It is a difference in kind, not in degree, of which the contrasted terms "deductive" and "inductive", "experimental" and "*a priori*" suggest the consequence, not the cause.

I have in the foregoing selected chemistry for contrast. But the choice is not necessarily limited to the natural or physical sciences. Psychology, ethics, politics, indeed any of the recognized fields of systematic study—except the purely speculative sciences, metaphysics and mathematics—pursue its scientific goal in approximately the manner indicated.

Accordingly, the proposition which I venture to maintain is that modern studies of economic distribution have neglected the method appropriate to positive science and have become, in large part, barren, unscientific, quasi-metaphysical speculations, standing in little better than what might be termed atmospheric relation to the concrete facts which they undertake to interpret; and consequently, that if the economist's search for a law of distribution is not to be merely an intellectual exercise in subtle dialectics, but a fruitful endeavor to ascertain the uniformities which obtain in economic distribution—his mode of inquiry must be a more deliberate study of his

subject-matter, approximating in so far the method of positive, instead of speculative science.

Such a contention does not resolve itself into an advocacy of induction, as against deduction, as the method which the economist should use in his search for a law of distribution. The old antithesis has largely passed away. Induction, as Professor Mackenzie has stated, "is the method by which the premises of the deductive method are secured, and by which its results are verified",¹ and Bagehot has pointed out, as Professor Keynes reminds us, that we can only proceed to a successful analysis of complex economic facts, by "a synthesis based on a previous examination of the nature and action of elementary forces, through whose operation the complex facts are produced."²

My contention is rather that the student of distribution has failed to make any conscious use at all of true induction. In so far as his method has purported to be inductive, it has been historical and comparative rather than analytical. In so far as it has been deductive, it has neither derived its premises nor verified its conclusions.

There are, I take it, three conceivable methods of investigation which the economist may employ in his search for a theory of distribution—using any one of the three exclusively or associating them in various proportions. For purposes of convenience I shall denominate these: 1. the historical; 2. the metaphysical; 3. the analytical. It is very possible that these phrases are inexact and that more precise nomenclature is desir-

¹ "Inductive method" in Palgrave (ed.), "Dictionary of political economy," vol. II, p. 392.

² Keynes, "The scope and method of political economy" (London, 1891), pp. 197-198.

able ; but it is with the several concepts rather than with the corresponding terms that my prime concern lies.

1. The historical method of determining the law of economic distribution is the modern phase of that momentous reaction against theoretical universalism and perpetualism in the social sciences which we trace back to Savigny, and associate more directly with Roscher, Hildebrand, and Knies. As a scientific procedure, it is perhaps better described as the "monographic method". Its devotees insist that every stage and every form of economic organization have had a corresponding theory of distribution, and that until these innumerable types have been depicted in detail and interpreted in outline, no attempt at a general law of distribution should be made.

The method is not an ungrateful one. "The mind," says Professor Sidgwick, "wearied of the vain effort to ascertain what is true, settles down more and more to the task of what has been held."¹ Yet whatever hope may have been entertained as to the positive utility of the historical method in constructing a body of economic doctrine was—it is now seen—born of reaction. Monographs have multiplied ; doctoral dissertations have accumulated, and the progress of the historical economist towards an admitted theory of distribution has been imperceptible. The experience of two score years seems to suggest that the prime usefulness of such minute studies is educational and local, and that variety of approach, distinctness of treatment, change of environment are grave qualifications, under existing conditions, of the value, and certainly of the economy, of large reliance upon this intensive method of inquiry.

¹ "Philosophy, its scope and relations" (London, 1902), p. 166.

2. The metaphysical method as applied to the theory of distribution is the orthodox *a priori* or deductive method brought down to date. It is not the classical form of speculation represented, for example, by geometry or pure philosophy, wherein starting from a limited number of fundamental propositions, either self-evident truisms or established principles derived from other domains of thought, the student passes step by step to particular applications by a pure and rigid logic. It is not quite the qualified deduction wherein the fundamental generalities are derived by a tentative, half unconscious induction from a loose and informal array of facts, and where the conclusions are from time to time referred to actual phenomena for favorable confirmation or for graphic illustration. It is rather a combination of criticism, speculation, and emanation. Its starting point is either the classical theory of distribution or the utilitarian calculus of pleasure and pain; its procedure is a dialectical criticism of abstract principles, and its equipment is an acute and refined rationalism, relieved by that plausible intimacy with the facts of modern industry which comes of common sense and professional sympathy.

A concrete example of this method is the ordinary procedure of the modern economic theorist. He will have been suckled on Ricardianism, weaned on John Stuart Mill, and fed on Fawcett or Sidgwick. Comte or Spencer or Roscher may have stirred him to methodological revolt, but there has been no positive contribution. Suddenly comes a doctrinal explosion superinduced from without,—Jevons and marginal utility, Walker and residual claimant, Böhm-Bawerk and esoteric interest, Clark and imputed productivity. Forthwith, our economist becomes dialectician whose further scientific

activity is a mean between closet speculation and metaphysical hypercriticism, bearing to the concrete facts of economic organization that tenuous and impalpable relation which a garment in autumn holds to the camphorated atmosphere in which it has rusticated.

The output of the method is enormous. Under provocation of a somewhat similar literature, Mr. James Bryce has said: "The difficulty which forbids many among us to give more study to these books is the shortness of life. Much talent sometimes of a high order, has gone to the making of them. But they are, and not solely the German ones, terribly hard reading."¹

Far be it to speak with a cheap and frivolous levity of what represents, in great part, an earnest and devoted effort to extend the bounds of human knowledge. But whether estimated by tangible result, by internal quality, or by analogy with other fields of systematic inquiry—such effort cannot be regarded as other than disproportionate or misdirected.

3. There remains to be considered the analytical method as the proper scientific instrument for the determination of the law of distribution. This method assumes that economic distribution, unlike the subject-matter of mathematics or metaphysics, is made up of facts of experience, and that the theory of distribution belongs not with the mental disciplines but with the doctrines of positive science. To be more specific, it defines the theory of distribution as the principle or body of principles, in accordance with which the product of industry is apportioned among the rightful claimants thereto. It asserts that the details of economic distribution are capable of observation, arrangement, and classi-

¹ "The methods of legal science" in "Studies in history and jurisprudence" (New York, 1901), p. 623.

fication; that when so organized, they reveal certain uniformities, that these uniformities may be formulated as hypotheses, and that these hypotheses or theories may be demonstrated as laws. In other words, that given the actual subject-matter of employment and remuneration, income and revenue—an interpretation is possible.

This is no new-fangled device. When we examine with some minuteness the history of the theory of distribution, it is palpable that the most important elements therein were consciously or unconsciously inductions from an immediate environment. The physiocratic net product and sterility of labor were economic interpretations of contemporary France. Adam Smith knew his industrial and commercial Britain as well as his philosophical heritage. Malthus, West, Torrens, and Ricardo took the law of diminishing returns from the parliamentary blue-books of 1813-15 on the condition of English agriculture and the effects of the corn-laws. Longe assailed the wage-fund theory after official duty on the Children's Employment Commission had brought him in contact with large employers of labor, and it was surely Walker's intimate acquaintance with the concrete facts of American industrial organization growing out of his service as director of the ninth and tenth federal censuses that led him to differentiate the entrepreneur from the capitalist as an economic functionary. Not until our own generation may it be said that new forces operated. John Stuart Mill's philosophical justification of deduction as the exclusive method of economic inquiry; the one-sided revolt of the historical movement from economic speculation to institutional rather than analytical inquiry; the reaction in turn from the barrenness of historical studies to constructive speculation, and finally the tremendous in-

crease in the extent and complexity of industrial phenomena—all these have encouraged the substitution of speculation for analysis, and in so doing have given a mischievous bent to economic inquiry as applied to the theory of distribution.

The detailed use of analysis as a scientific method is a commonplace to every student of positive science. In an admirable essay on the progress of science in the Victorian era, Mr. Huxley has summarized this procedure with respect to study of natural phenomena, but the exposition is applicable with slight modifications to the domain of psychology, ethics, politics, and economics. "The development of every branch of physical knowledge presents three stages, which, in their logical relation, are successive. The first is the determination of the sensible character and order of the phenomena. This is *natural history*, in the original sense of the term, and here nothing but observation and experiment avail us. The second is the determination of the constant relations of the phenomena thus defined, and their expression in rules or laws. The third is the explication of these particular laws by deduction from the most general laws of matter and motion. . . . Historically, no branch of science has followed this order of growth; but, from the dawn of exact knowledge to the present day, observation, experiment, and speculation have gone hand in hand."¹

The analytical investigator must suffer a similar experience in his search for a theory of distribution. He must collect his facts, he must formulate his theories, and he must correlate the theories of distribution so at-

¹"The progress of science, 1837-1887," in "Methods and results: essays" (New York, 1898), pp. 64-65.

tained with his general economic system or social philosophy.

Of the three phases, it is easy to see wherein lies the present need. Speculative theorization we have had galore, and ambitious systematization hardly less. The primary requisite of the analytical method at this particular juncture is systematic observation, a natural economic history—not an economic history as commonly understood, a record of past economic life and growth; but more properly, an economic survey or description, a transcription of contemporary economic happenings in so far as they are the subject-matter of the doctrine under investigation.

Perhaps no period in the history of our science has been so relatively uninformed as to the essential aspects of its economic environment as our own. I do not ignore the activity of modern economic inquiry, nor the range and extent of modern statistical enumeration. But as far as statistics are concerned, we have been given quantitative aggregates and not qualitative details. With respect to the results of modern economic inquiry, the deficiencies are even more glaring. Physical conditions in the United States invited, and the German historical movement justified, economic microscopics. In consequence, although pursued with unexampled activity, economic investigation in the United States has been almost exclusively historical or institutional, on the one hand, and local and intensive on the other. Of extensive economic investigation, economic description in the proper sense of the term, little has been attempted and less achieved. The historical evolution of economic institutions as revealed in more or less accessible records, the functional activity of economic organization as displayed in limited areas—these have defined

the scientific activity of the ordinary economist. Of the comprehensive study of the structure and function of any actual part of the present economic organization, we have had infrequent example. It is not difficult to lay a finger on the causes of this neglect. Mr. Huxley has said "observation and experiment are hard work, while speculation is amusing."¹ It is much easier to spin cobwebs about the theory of distribution in the closet, than to describe the present, the actual facts of distribution in the field. I do not mean to neglect the effect of Mill's, Senior's, and Cairnes' methodological teaching—summed up best perhaps in Senior's dictum that political economy "depends more on reasoning than on observation."² I recognize that the vast area, the extensive activities, and the scattered data subject to economic inquiry, on the one hand, and the inadequate equipment of the economic investigator, both as to resources and opportunities, on the other hand—have imposed a heavy handicap upon comprehensive economic description. But withal the student of distribution has pursued the speculative course largely because it represents the line of least resistance. Brought face to face with large subject-matter, we have shown the white feather and solaced our souls in the futile thought that extensive description might properly be postponed until such a number of detailed monographs, dealing with specific aspects of the subject, have been completed as will permit full exposition and safe generalization.

I have in mind no rigid Baconian method ; no prosecution of scientific inquiry as a manner of formalized book-keeping, where in Jevons' phrases—"facts were

¹ *Loc. cit.*, p. 66.

² Cf. Keynes, "The scope and method of political economy" (London, 1891), pp. 16-17.

to be indiscriminately gathered from every source, and posted in a kind of ledger, from which would emerge in turn a clean balance of truth.¹” I do not exalt any one formal method as a scientific nostrum, neglecting the fact that many paths lead to the central truth and that the investigator is certain to select that course and that equipment best suited to his needs and capacity. I note merely the absence of the necessary material of the student of distribution, which if proceeding along deductive lines he requires for his initial synthesis and for his ultimate verifications; or if along inductive lines, for his essential subject-matter,—that large accurately described, well digested body of facts, absolutely indispensable, whether after the teaching of Bacon to make theories from, or in the light of the history of scientific discovery to try ready-made theories by.

I propose, therefore, an arrest of further text-book didacticism, an abandonment of hypercritical dialectics, and a reduction to a minor rôle of metaphysical speculation as the dominant apparatus serviceable in the search for the law of economic distribution. Measurably in lieu thereof, and largely in connection therewith, I venture to suggest a detailed and systematic acquaintance with the actual contemporary subject-matter of economic distribution, as the preparation and equipment for its scientific interpretation.

Ideally, this would involve an economic description of the United States in the year of grace 1906 in which every phase of contemporary industrialism in so far as affects economic distribution, would be faithfully and systematically transcribed. Trade unionism, methods of industrial remuneration, entrepreneurs' gains, in-

¹ Bagehot, “The postulates of English political economy” in “Works.” (Hartford, 1891), vol. V, p. 250.

dustrial combinations, corporate organizations, business risks and failures, interest rates, banking institutions and commercial discounts, land tenures, urban rents—all these would be depicted, not in their historical development or local manifestation, but in their contemporary phase and in their widest extension. I pass by the intrinsic interest of such a photograph of contemporary economic life, and view it merely as scientific data. The economist of the past generation could omit a systematic round up of subject-matter, because the facts of distribution were incomparably simpler. It was possible for a keen observant mind to grasp without deliberate canvass the essentials of its economic environment. Adam Smith knew his Glasgow to the core, and Glasgow was the economic world in microcosm. Even McCulloch, the economist, was McCulloch, the author of the "Commercial dictionary". But the economic life of today in its range and complexity has utterly passed beyond the mental compass of the casual onlooker—be he economist or man of affairs, and unless the equipment is to be inadequate and the perspective distorted, there must be deliberate collection and report.

I am not insensible of recent tendencies in the direction indicated; nor of the particular studies of contemporary economic life that loom up here and there in solitary isolation in the output of the American economist in the past generation. But surely the general proposition remains unaffected. Our subject-matter has outrun our systematic knowledge of it, and largely in consequence of deliberate neglect, we are relatively uninformed as to the essential data which we are seeking to interpret.

As to a practical proposal! Is the student of distribution to postpone his thinking until the facts have been

gathered—to throw away his crucible and to take to a shovel? Certainly not. That was the fault and the failure of the historical school. Here again Mr. Huxley is guide and prophet: “Whenever science has halted or strayed from the right path, it has been, either because its votaries have been content with mere unverified or unverifiable speculation—; or it has been, because the accumulation of details of observation has for a time excluded speculation.”¹ If we have wandered too far to the right, correction does surely not involve excessive divergence to the left. The true course lies in that union of observation and speculation which the progress of every science vindicates. That union which we economists have blandly proclaimed, and certainly in the study of economic distribution have coolly neglected.

In accordance with this view, the theory of distribution will be sought, not necessarily from the facts, but through the facts. It would, for example, have the theory of wages studied not exclusively by doctrinal criticism nor *a priori* speculation, but by investigation of existing wage relations. A theory of entrepreneurs’ profits should be based upon a no less intimate knowledge of contemporary business management than distinguished Francis A. Walker’s analysis a generation ago. The student of capital and interest might properly turn from the irrepressible Böhm and search for a clue in the practice and technique of the money market. Urban rents and agricultural conditions no less than social marginal disutility are certainly the substance of a rent theory.

In short it seems not unreasonable that the author of a general theory of distribution should have as a basic equipment an intimate knowledge of the nature and technique of the practice of distribution. I have else-

¹ “The progress of science,” *loc. cit.*, p. 65.

where sought to make clear the three essentials for such procedure.¹ First, the economic investigator must be able to command, in addition to ordinary library apparatus, all primary documentary material relevant to his inquiry, whether it is as ephemeral as municipal reports and trade-union journals, or as unobtainable by formal request as trade agreements and corporation records. Second, the investigator's time and energy, if not entirely available for scientific inquiry, must certainly not be unduly absorbed by the routine engagements of the student or the teacher. Third, with respect to resources, the investigator must be in command of funds sufficient to enable him to visit and upon certain occasions temporarily to reside in representative localities for the purpose of gathering additional evidence and of testing and verifying tentative conclusions. "Investigation funds" must be regarded as essential to scientific activity in political economy as laboratory apparatus is to chemistry and clinical provision to medicine.

It by no means follows that such an acquaintance with facts will inspire a theory, any more than it is mathematically certain that a theory will emerge in no other way than by study of facts. Perhaps the divine spark that in all times has flashed to light dark places will come when least awaited—even when least deserved. The history of science teaches that the prize is not always to the investigator who toils the hardest nor the longest. But it will never descend—and surely never re-descend—upon him who gropes blindly or uninformed.

¹"The political economist and the public," in *North American Review*, February, 1905.